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Preoperative body mass index and recorded hernia recurrence in adults with diabetes in Riyadh: a retrospective cohort reanalysis

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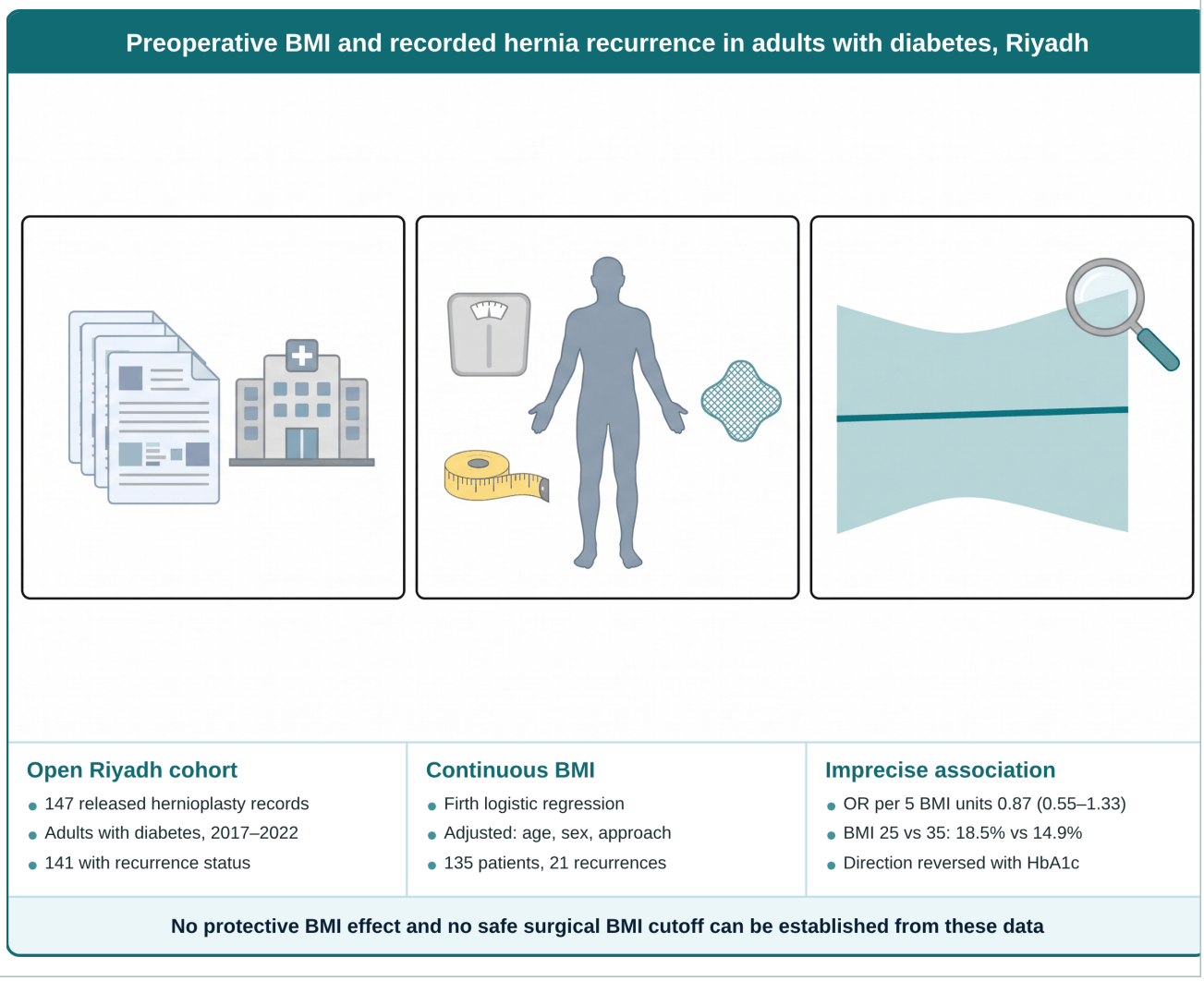
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Abstract

Graphical abstract



Preoperative body mass index (BMI) is frequently considered during assessment for hernia surgery, but a single threshold can obscure differences in operative approach, patient selection and outcome ascertainment. We reanalyzed an openly released retrospective cohort of adults with diabetes treated in Riyadh, Saudi Arabia, to estimate the association between continuous BMI and recorded postoperative recurrence. A frozen analysis plan specified Firth logistic regression adjusted for age, sex and surgical approach, with profile-likelihood intervals and bootstrap uncertainty for standardized risks. Of 147 released records, 141 had recurrence status; the primary model included 135 patients and 21 recurrences. The adjusted odds ratio per five-unit higher BMI was 0.87 (95% confidence interval 0.55–1.33). Standardized recurrence probabilities at BMI 25 and 35 were 18.5% and 14.9%, respectively; the difference was -3.6 percentage points (95% interval -16.2 to 5.3). The estimate moved across the null after additional glycemic-category adjustment. Missing follow-up dates, absent hernia subtype and differences between the public file and published denominators constrained interpretation. These data do not establish either a protective BMI association or a safe surgical threshold. They support cautious interpretation of small local cohorts and better prospective documentation of perioperative risk and recurrence ascertainment.

Keywords: body mass index; hernia recurrence; perioperative assessment; diabetes; Saudi Arabia; retrospective cohort; reproducibility

Introduction

Preoperative assessment for abdominal wall surgery often brings together several related questions: whether the proposed operation is appropriate, whether medical optimization is possible, and how the patient should understand the probability of subsequent complications. Body mass index is easy to measure and is commonly included in this discussion. Its accessibility does not make it a complete representation of operative risk. Body composition, glycemic status, functional capacity, the anatomical defect, previous repair, surgical technique and postoperative events can all influence interpretation. A risk estimate that omits these features should therefore be used to describe the population actually observed, with uncertainty that reflects the available information.

The published literature does not reduce the BMI–recurrence relationship to one universal cutoff. A systematic review identified multiple patient and procedural predictors of ventral hernia recurrence, while a separate review specifically addressed minimally invasive repair [3–5]. Surgical-site occurrences have also been examined as determinants of later recurrence [8]. These bodies of evidence distinguish long-term recurrence from short-term morbidity, and distinguish individual techniques from the broad label of hernia surgery. Guideline recommendations on abdominal wall surgery and incision closure similarly place risk assessment within a wider clinical and technical context [9–12]. Their recommendations cannot be tested by a small dataset that does not retain hernia subtype or repair details.

Saudi evidence is relevant because referral pathways, the population selected for surgery and subsequent access to follow-up may differ from those of international registries. A previous Saudi cohort evaluated BMI together with other recurrence-related factors at King Abdulaziz University Hospital [6]. That cohort was from a different center and does not establish the behavior of every surgical population in Saudi Arabia. A separate openly deposited Riyadh dataset was originally used to study categories of preoperative HbA1c in adults with diabetes [1,2]. It provides an opportunity to examine an additional, focused question using a measured preoperative exposure. The public release also permits assessment of which denominators and variables can actually be reproduced.

We asked whether preoperative BMI, retained as a continuous measure, was associated with recorded recurrence after hernioplasty in this Riyadh cohort after adjustment for age, sex and operative approach. The intention was an associational secondary analysis, not development of a clinical prediction score or an evaluation of intentional weight loss. We prespecified sparse-event estimation, an absolute-risk presentation and sensitivity analyses for missingness, glycemic status and extreme BMI values. Because recurrence dates and complete follow-up information were unavailable, we treated the released recurrence indicator as an observed record-based endpoint and did not manufacture a survival analysis.

Methods

Design, source and setting

This was a secondary retrospective cohort analysis of a publicly released, de-identified workbook. The source report describes adults with diabetes undergoing hernioplasty at King Abdulaziz Medical City in Riyadh during September 2017–September 2022 and defines recurrence through documented clinical diagnosis during postoperative follow-up [1]. The workbook was downloaded from Figshare, version 1, under CC0 terms [2]. The analysis used the released file rather than extracting individual values from publication tables. The original bytes, file hash, repository metadata and transformation script accompany the analytic dataset.

The source publication addresses HbA1c categories and reports a smaller analytic denominator than the number of rows in the released workbook. We did not select records to recreate that denominator without a reproducible selection rule. Instead, eligibility was determined separately for the recurrence endpoint and the variables required by each prespecified model. This distinction is material: a complete-case population depends on which variables are being studied. Exact duplicates were checked, but records would not have been removed merely because a limited set of clinical characteristics happened to coincide. No patient identifier was supplied for record linkage.

The source article was used to establish the clinical setting and intended variable definitions; the workbook was used to determine the records actually analyzed. These roles were kept separate. A published percentage could not override a released patient-level code, and agreement with one published marginal count was not treated as proof that the same patients had been selected. The recurrence denominator excluded missing recurrence alone before covariate completeness was considered. This preserves an auditable distinction between absence of an endpoint and absence of a proposed explanatory variable. It also allows a later custodian correction to be incorporated through a new, explicitly versioned analysis rather than an undocumented change to the present results.

Time zero, exposure and outcome

Surgery was the clinical index event. BMI was the preoperative value recorded in the public data, expressed in kg/m². The primary parameter compared the odds of recorded recurrence for a five-unit higher BMI, under a linear BMI specification. BMI was centered for numerical stability; centering does not change the slope. No threshold was selected from the observed outcome data, and obesity categories were not substituted for the continuous primary exposure.

The primary endpoint was the released binary recurrence code. The source describes a postoperative recurrence assessment within one year, but the public file contains no dates for recurrence, censoring, death or the last clinical assessment. Thus a negative code cannot independently establish equally complete surveillance in every patient. The analysis estimates an association with recorded recurrence rather than a censoring-adjusted one-year cumulative incidence. A Cox model, competing-risk curve or immortal-time correction would require information that is not present. Postoperative length of stay is not a substitute for the recurrence follow-up interval.

The additional wound-infection variable explicitly refers to the first postoperative month. It was retained for descriptive context with an exact binomial interval. It was not included as a primary adjustment variable because it occurs after the exposure and surgery, and could lie on a pathway involving operative events. Nor was a sparse infection model used to generate a second independent clinical claim. Recurrence and infection were not combined into an unvalidated composite.

Covariates and coding

The primary adjustment set comprised age, sex and open versus laparoscopic repair. These are preoperative or procedural characteristics that can be relevant to both the operative population and the recurrence recorded subsequently. Age was represented per decade without a data-selected age cutoff. The release labels sex as male or female; no information about gender identity was available. The workbook's separate coding sheet was used to interpret categorical values, including the coding direction for surgical approach and the two HbA1c categories.

HbA1c was available only as a category. Actual concentrations, their measurement dates and the handling of a value exactly at the category boundary were not recoverable. Accordingly, glycemic adjustment was a prespecified sensitivity model on records with an observed category, and was not described as adjustment for continuous glycemia. The release did not include hernia subtype, defect dimensions, mesh use or mesh position, duration of diabetes, postoperative glucose, operative complexity, surgeon, medication treatment or recurrence-detection method. Hypertension, kidney disease and smoking were present but were not added through outcome-driven covariate selection. The small number of recurrences limited the information available for a large model.

Analysis plan and model estimation

The analysis plan was written and hashed after inspection of variable names, coding, missingness and marginal feasibility counts, but before any BMI–recurrence cross-tabulation or model fitting. It was not prospectively registered. The known results of the original HbA1c study were available when planning this secondary analysis. The primary and sensitivity specifications were retained irrespective of the direction or statistical significance of the BMI estimates.

Firth logistic regression was selected to reduce small-sample bias and accommodate sparse covariate patterns [21]. The objective was the binomial log likelihood plus one-half the log determinant of the expected information matrix. The primary model included an intercept and the prespecified exposure and adjustment variables. The BMI odds ratio was reported with a profile penalized-likelihood confidence interval. This interval avoids relying solely on a symmetric normal approximation on the coefficient scale. The penalty addresses a statistical estimation problem; it does not correct unmeasured confounding or inadequate recurrence ascertainment.

To make the conditional odds ratio more interpretable, we estimated model-standardized probabilities at BMI 25,30,35 and 40. For each grid value, the BMI term was replaced in every primary analytic record while the observed distribution of other covariates was retained. The fitted probabilities were then averaged. The prespecified absolute contrast was BMI 35 minus BMI 25. These quantities are standardized associations under the fitted model, not the effects of changing an individual's weight. A patient bootstrap refitted the model and restandardized over the resampled covariate distribution; percentile intervals describe sampling uncertainty under that procedure.

Missingness and sensitivity analyses

The primary model used complete records for its required variables. The missing-outcome proportion was described separately from missing covariates, and the smaller denominators in sensitivity models were reported. Complete-case estimation can be biased if availability depends on both exposure and outcome or their determinants. A missing category for operative approach was therefore used only as a sensitivity analysis, without claiming that it recovers the information in the original surgical records.

We also fitted an unadjusted model, a model additionally adjusted for HbA1c category, and a model restricted to BMI at or below 50. A high BMI was not declared erroneous simply because it was uncommon. The restriction was used to assess dependence on extreme observations. An exploratory restricted cubic spline with three prespecified percentile knots assessed whether the linear form concealed obvious curvature. Its greater flexibility was not used to select a favorable result or derive a clinical cutoff.

Missing recurrence status was examined using transparent extreme-case bounds within BMI below 30 and BMI at least 30. The lower bound counted every missing endpoint as no recurrence; the upper bound

counted every missing endpoint as recurrence. These bounds do not solve selection or follow-up bias, but show the direct contribution of the unrecovered outcomes without a model-based imputation. Multiple imputation was not presented as a remedy for absent follow-up dates or undocumented outcome ascertainment. Its assumptions would need stronger justification than the available release provides [22].

Verification and reporting

The direct likelihood optimizer was checked using a separately written score-equation solver. The verification implementation used QR-based leverage calculations and reopened the raw workbook independently. It also evaluated the confidence-interval endpoints using a separate nuisance-parameter optimizer and a Cholesky determinant calculation. Counts, coefficients and standardized probabilities were compared against the results lock. Bootstrap confidence intervals were not independently regenerated in that second lane; this limitation is stated in the numerical verification receipt.

Reporting was mapped to STROBE cohort items and the applicable RECORD extensions [23,24]. The data package records source provenance, exclusions, coding, missingness, the frozen plan, numerical results, source tables for the figures and executable scripts. No matching, propensity procedure, event dates or clinical adjudication were asserted when none was present. The claims were limited to associations in the released cohort.

Results

Available records and analytic population

The release contained 147 records, with no exact duplicate rows. Recurrence status was observed in 141 records, including 23 recorded recurrences. The primary model included 135 patients and 21 recurrences after excluding 6 missing recurrence outcomes and a further 6 records with missing operative approach among those with known recurrence status (Figure 1). Age, sex and BMI were complete in the release. Missingness remained for glycemic category, operative approach and other variables; each model therefore has its own denominator.

The mean age in all released records was 58.0 years (standard deviation 11.3), and median BMI was 31.23 (interquartile range 26.96–36.02). BMI ranged from 15.23 to 63.30. The primary cohort included 71 male patients and 37 open procedures (Table 1). The available records were predominantly selected surgical patients with established diabetes; they do not represent all people referred for hernia assessment or all patients whose surgery was deferred.

The released workbook contains 147 rows, whereas the source article reports 140 participants in its main analytic cohort [1,2]. Recurrence was coded positively in 23 released records with an observed endpoint. Restriction to records with observed operative approach left 21 recurrences in the primary model, but that numerical agreement with the source article's recurrence count does not identify the source article's 140-person cohort. The current primary denominator is 135. Missingness also differed across variables: the release had eight missing procedure values and ten missing HbA1c categories. These counts show why one common denominator cannot be assigned to every analysis. No additional records were removed to reproduce a previously reported table.

Table 1. Available records and primary analytic cohort.

Characteristic	All released	Primary analytic
Records	147	135
Age, mean (SD), years	58.0 (11.3)	58.1 (11.3)
BMI, median (IQR), kg/m²	31.23 (26.96–36.02)	30.93 (26.89–35.95)
Male, n	76	71
Open repair, n / observed	37 / 139	37 / 135
Uncontrolled HbA1c category, n / observed	89 / 137	82 / 125
Recurrence, n / observed	23 / 141	21 / 135

BMI, body mass index; IQR, interquartile range; SD, standard deviation. Denominators vary because missingness is endpoint- and variable-specific. HbA1c is a category, not a released laboratory concentration. Table values are generated from RESULTS_LOCK.json.

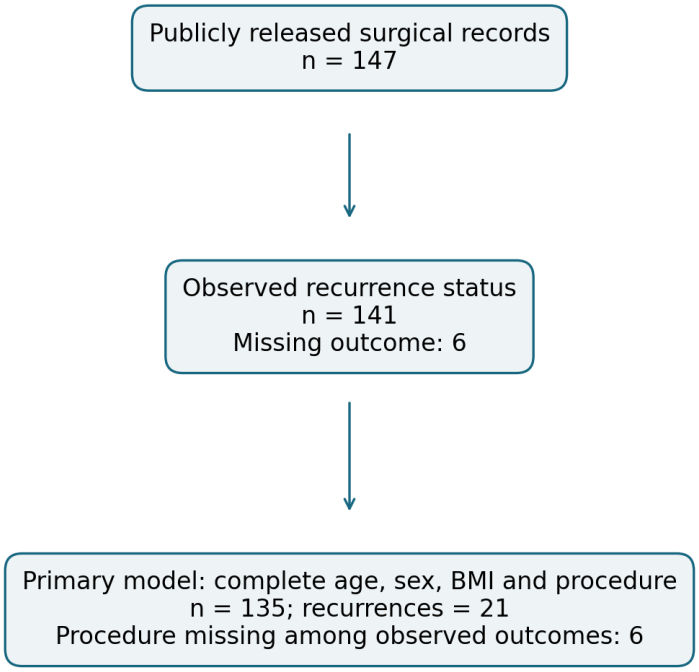


Figure 1. Cohort assembly from the public release.

Counts show sequential exclusions for missing recurrence and missing procedure among records with recurrence observed. No undocumented exclusions were applied to force the source publication denominator. The primary model uses complete age, sex, BMI and operative approach.

Primary BMI association and absolute probabilities

The adjusted odds ratio per five-unit higher preoperative BMI was 0.87, with a 95% profile interval of 0.55–1.33. This interval included both lower and higher odds of recorded recurrence. The fitted direction should

not be interpreted as a protective association. The model converged with an adjusted-score maximum of 1.05e-09; the smallest information eigenvalue was 1.87, and the largest leverage was 0.181.

Standardized recurrence probabilities at BMI 25,30,35 and 40 were 18.5%, 16.6%, 14.9% and 13.3%, respectively. The BMI 35-versus-25 contrast was -3.6 percentage points, with a bootstrap interval from -16.2 to 5.3 percentage points. All 1000 prespecified bootstrap fits produced finite converged estimates. The observed central BMI support and wide intervals are shown with the fitted estimates below; the grid is an explanatory display, not a treatment recommendation.

Sensitivity estimates and outcome availability

The unadjusted odds ratio was 0.76 (0.52–1.07). Adding HbA1c category reduced the analytic sample to 125 patients with 18 recurrences and yielded an odds ratio of 1.04 (0.63–1.74). Because both the adjustment set and the analyzed records changed, the difference between these estimates cannot be attributed exclusively to glycemic confounding. The BMI-restricted and missing-procedure analyses were also compatible with no association (Table 2; Figure 2). The exploratory spline was used to display functional-form sensitivity without establishing a threshold (Figure 3).

Table 2. BMI association under prespecified model and cohort specifications.

Specification	N	Events	OR per 5 kg/m² (95% CI)
Unadjusted	141	23	0.76 (0.52–1.07)
Primary: age, sex, procedure	135	21	0.87 (0.55–1.33)
Primary plus HbA1c category	125	18	1.04 (0.63–1.74)
BMI <=50	134	21	0.87 (0.55–1.36)
Missing procedure category	141	23	0.91 (0.58–1.37)

All odds ratios use Firth logistic regression and profile penalized-likelihood confidence intervals. The primary model adjusts for age, sex and procedure. Additional HbA1c adjustment changes both covariates and the analyzed records. CI, confidence interval; OR, odds ratio. No estimate is interpreted causally.

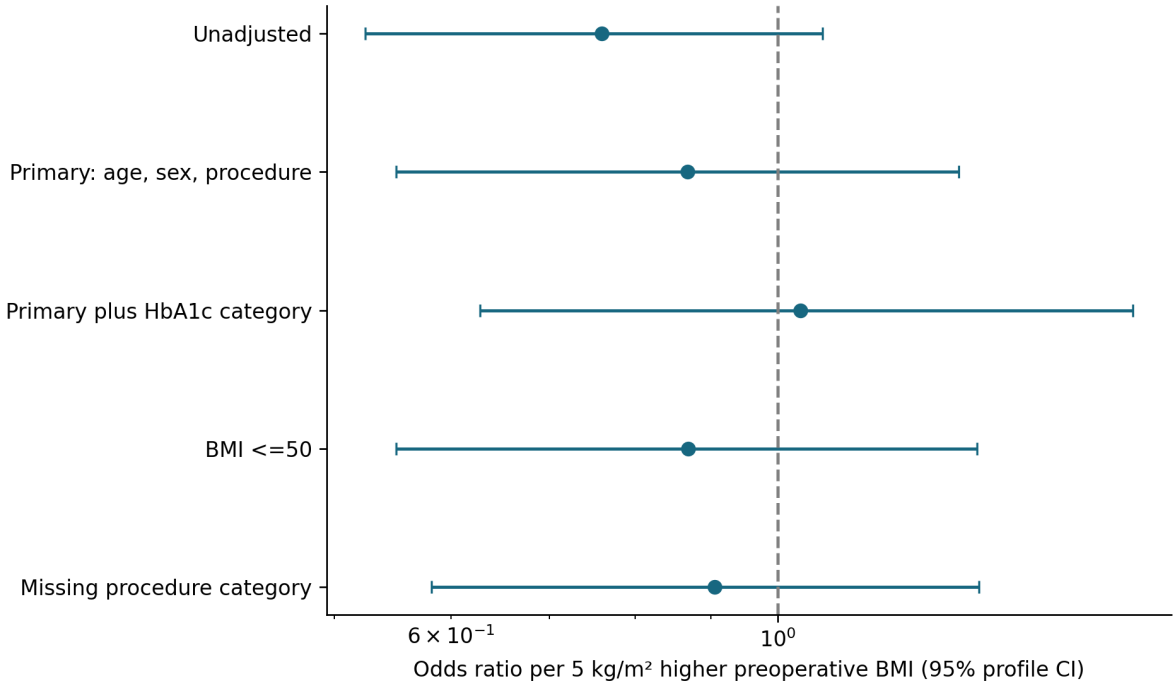


Figure 2. BMI association across primary and sensitivity analyses.

Points represent odds ratios per 5 kg/m² higher preoperative BMI; horizontal bars are 95% profile penalized-likelihood intervals. The dashed reference denotes equal odds. All intervals include that reference; the direction changes in the glycemic-category model. The log-scaled axis displays relative associations, not treatment effects.

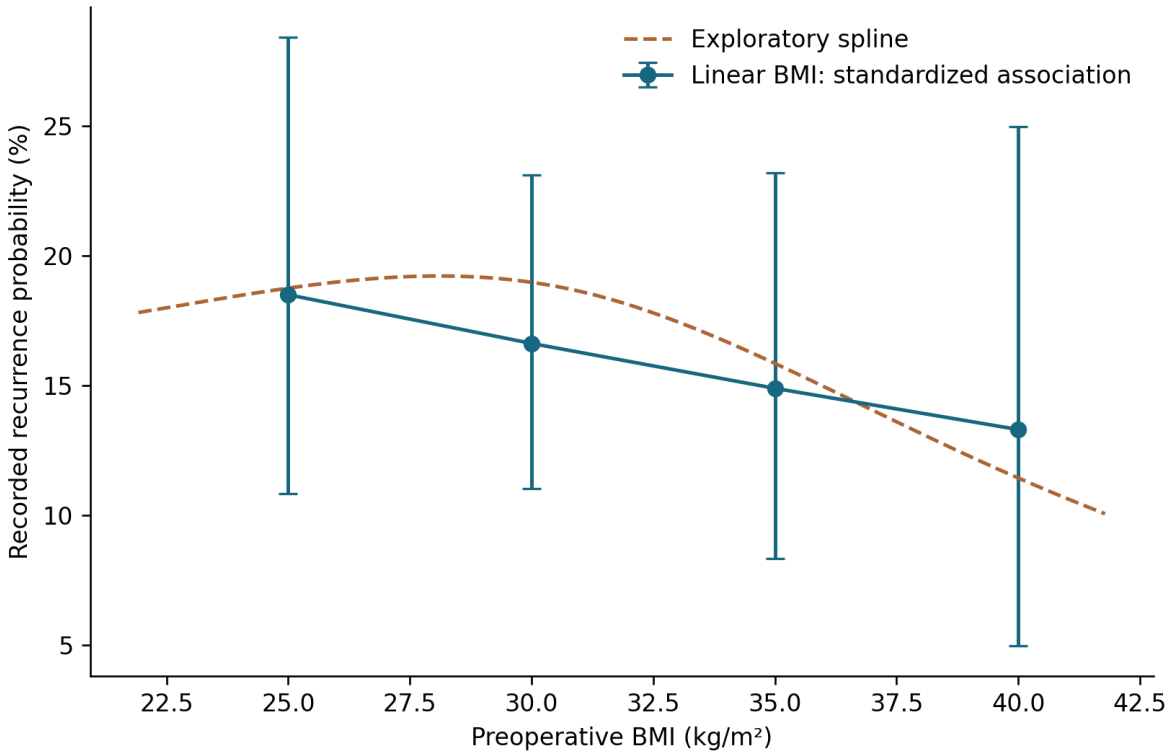


Figure 3. Standardized recurrence probabilities and functional-form sensitivity.

Points average the primary model probabilities over its observed covariate distribution at each displayed BMI. Bars show percentile intervals from the locked patient bootstrap. The dashed exploratory spline uses

the same adjustment variables. The curves do not estimate the effects of intentional weight change or identify a safe surgical cutoff.

Among records with BMI below 30, the extreme-case recurrence bounds were 19.0% to 20.6%; the corresponding bounds at BMI at least 30 were 13.1% to 19.0%. These are unadjusted bounds for the released rows, not confidence intervals. Wound infection was recorded in 8 of 143 patients with an observed infection code (5.6%; exact 95% interval 2.4%–10.7%). The infection endpoint remained descriptive.

Discussion

Principal interpretation

This secondary analysis did not provide a precise estimate of a BMI–recurrence association in adults with diabetes undergoing hernioplasty in Riyadh. The primary adjusted interval spanned both lower and higher recurrence odds, and the standardized probability difference was similarly imprecise. Additional adjustment for the available glycemic category changed the direction of the point estimate while also changing the analytic population. These findings argue against interpreting the fitted inverse slope as protection from higher BMI. They also do not establish absence of clinically important risk. A null-compatible result in a limited cohort is an uncertainty statement, not evidence that preoperative risk assessment can disregard weight-related factors.

The most useful contribution is the separation of a focused exposure question from the information the data actually retain. The release permits a reproducible association analysis using recorded BMI, recurrence and a small adjustment set. It does not permit a causal evaluation of weight reduction, comparison of surgical timing strategies, or adjustment for all the anatomical and technical determinants of recurrence. Reporting that boundary explicitly is more informative than attaching an elaborate survival model to a file without survival times. The use of penalized estimation and independent numerical checks improves statistical reliability within the observed data but does not remove their clinical limitations.

Relationship to the wider surgical evidence

Published investigations differ in whether they study primary or recurrent hernias, ventral or inguinal defects, open or minimally invasive repair, and short-term complications or later recurrence [3–8,13,15,18–20]. Such differences affect the meaning of a BMI estimate. For example, an association with perioperative morbidity need not have the same magnitude or even the same confounding structure as an association with documented recurrence. The minimally invasive evidence and analyses emphasizing surgical-site occurrences illustrate why broad BMI statements should not be transferred without considering operative technique and endpoint [5,8]. The present mixed-procedure release cannot resolve those sources of heterogeneity.

Several published studies have examined thresholds or operational consequences of BMI-based selection, including hospital readmission after ambulatory hernia surgery [7,16,17]. A cutoff extracted from one population is not automatically a biologic boundary in another. Threshold analyses can also be sensitive to the distribution of BMI, outcome prevalence, follow-up and the criterion used to choose the cutoff. Our continuous exposure model avoided searching for such a breakpoint. The exploratory spline was intended to reveal obvious sensitivity to the linear assumption; the limited recurrence information does not support a new surgical acceptance threshold.

Diabetes adds another layer of complexity. Evidence linking diabetes with surgical-site infection and studies of glycemic control in abdominal wall reconstruction address related but nonidentical endpoints [14,25]. In this dataset, diabetes was a feature of cohort membership rather than a comparison exposure. Restriction to operated patients with diabetes can reduce generalizability and create selection patterns different from an unselected surgical registry. A categorized HbA1c field only partly represents metabolic status. It cannot capture disease duration, perioperative glucose variation, treatment intensity or the clinical decision-making that preceded surgery.

The Riyadh source dataset and the previous Saudi BMI investigation also should not be treated as interchangeable local replications [1,2,6]. They involve different centers and eligibility decisions. The availability of a Saudi dataset supports direct relevance to an observed Saudi clinical setting, but not a national claim. The appropriate inference is confined to the selected records and their documented outcomes. A larger multicenter Saudi cohort with harmonized anatomical definitions and follow-up would address transportability more directly than pooling incompatible local estimates solely because they share a country label.

The distinction between the original HbA1c question and the present BMI question is also important when judging the added value of reanalysis. We did not relabel the published glycemic comparison as an independent new cohort. The contribution is a continuous BMI estimand with a prespecified limited adjustment set, absolute standardized probabilities, sparse-event estimation and an explicit audit of analyzable denominators. It uses the same released patients and should be cited as such. Agreement or disagreement with the original glycemic result is not a replication test because the exposure, adjustment and endpoint-complete samples differ. This dependence should remain visible in any later evidence synthesis so that the participants are not counted twice as separate cohorts.

Selection, ascertainment and missing data

The source publication's reported sample cannot be recreated by simply counting rows in the released file. Endpoint-specific missingness is therefore a central feature of this reanalysis. We retained a transparent flow from released records to observed endpoints and the primary covariate-complete sample. We did not assume that the unpublished selection rule was irrelevant, nor that the public file necessarily reproduces the exact population used in every source table. The discrepancy should be resolved with the data custodian before the dataset is used to support a definitive clinical claim.

Missing recurrence and incomplete follow-up are separate problems. The extreme-case bounds address the former within the available records. They cannot establish whether patients coded as having no recurrence were assessed at comparable times or whether some sought care elsewhere. Because dates and losses to follow-up are absent, censoring cannot be modeled and competing deaths cannot be distinguished from incomplete surveillance. A negative recurrence code is a documented data value, not proof of lifelong freedom from recurrence. Longer-term registry evidence underscores why the follow-up horizon matters when interpreting recurrence [18,20].

Missing operative approach and missing HbA1c likewise cannot be assumed to be random. The additional-glycemia model uses fewer records and its altered estimate combines sample selection with adjustment. A missing-category sensitivity preserves more observations but does not restore an unknown surgical technique. Multiple imputation would require assumptions about the relationships among missingness,

exposure and outcome; it would not reconstruct absent hernia anatomy or event dates [22]. We therefore present these analyses as sensitivity checks and retain the missing-data limitations in the interpretation.

Selection into the operative cohort is another potential source of bias that cannot be measured by the missing-value counts. For example, if both BMI and an unrecorded marker of surgical complexity influence whether surgery proceeds, restricting analysis to operated patients can associate BMI with that marker even before recurrence is considered. If the marker also predicts recurrence, an inverse observed BMI association may emerge without any protective biological effect. This is a possible explanation, not a finding established by the present data. The release contains no denominator of evaluated but unoperated patients, so it cannot distinguish such selection from other explanations. It would therefore be inappropriate to extend its fitted probabilities to all patients attending a preoperative clinic.

The missing-outcome bounds have a narrower role. Their endpoints describe what happens when the six explicitly unrecorded recurrence outcomes are assigned in opposite ways within broad BMI groups. They leave all recorded negative outcomes unchanged. Consequently, relatively narrow bounds would not validate the completeness of the original follow-up, and overlapping bounds would not amount to an adjusted test of an exposure effect. Using the bounds alongside the model makes two forms of uncertainty visible: incomplete coding within the released rows and imprecision under the fitted model. Neither calculation quantifies unrecognized recurrences outside the source institution. This distinction should be preserved when explaining the results to clinicians or designing an improved registry.

Statistical strengths and limits

The statistical workflow had several strengths: an analysis plan fixed before BMI–outcome modeling, retention of continuous BMI, bias-reduced estimation, profile intervals, an absolute-probability display and complete release of the numerical inputs. The model used a deliberately limited adjustment set rather than selecting variables by univariable significance. Independent score-equation fitting reproduced the main coefficient and standardized probabilities, and an independent profile calculation checked the confidence-limit boundaries. This makes numerical mistakes less likely and allows the results to be challenged directly.

Those checks do not create information that the sample lacks. The number of recurrence events remains small relative to the potential clinical determinants, so even an apparently stable fitted coefficient can be sensitive to omitted variables or a different sampled population. The bootstrap addresses sampling under the observed empirical distribution; it does not propagate uncertainty in the original abstraction process or unrecorded follow-up. The spline has additional parameters and must be regarded as exploratory. No significance threshold was used to upgrade one sensitivity model into the main result.

Surgical approach deserves particular care. The adjustment estimates an association conditional on recorded open versus laparoscopic repair. If BMI influences the choice of approach, conditioning on approach changes the clinical question from an overall BMI association to one within the modeled procedure distribution. The release cannot distinguish selection of approach from technical details within it. Consequently the model is useful as an adjusted description rather than a causal decomposition. The procedure coefficient itself was not promoted into a separate treatment-effect claim.

The odds ratio and standardized probabilities also answer different descriptive questions. The odds ratio summarizes a constant conditional BMI slope on the log-odds scale under the selected model. The standardized probabilities average fitted values across the covariates of this particular complete-record cohort. They are consequently sensitive to its distribution of age, sex and surgical approach. They should

not be inserted directly into a clinic's patient-information material without assessing whether its case mix and outcome ascertainment are comparable. Moreover, an odds ratio is not a risk ratio, and the absolute probability contrast depends on the underlying recurrence frequency. Presenting both scales aids interpretation without making either quantity transportable by itself.

Bias reduction does not guarantee calibration of the displayed probabilities in a new population. Firth estimation changes the fitted likelihood to stabilize estimation in limited samples; the profile interval remains conditional on the specified model and observed data structure [21]. The independent solver check establishes agreement between numerical implementations, not confirmation that the model is biologically complete. The same distinction applies to convergence of every bootstrap replicate: successful computation does not guarantee that the empirical sample represents all clinically relevant patient patterns. No external validation sample was available, and neither discrimination nor clinical utility was claimed. Those would require a separate prediction objective and an appropriately designed evaluation.

Implications for perioperative research

For anesthesiology and preoperative services, this analysis supports a restrained use of small local observational datasets. BMI can be included in risk discussion without allowing one imprecise coefficient to determine surgical eligibility. The study does not evaluate a weight-loss intervention, does not assess airway or cardiopulmonary complications, and does not compare anesthetic techniques. Any decision about delaying surgery or altering perioperative management must draw on the broader clinical assessment and the relevant guidance, rather than on the direction of this isolated association [9–12,17].

A more informative local research pathway would retain the complete preoperative assessment, hernia type and dimensions, prior repairs, mesh and operative technique, glycemic measurements with dates, standardized wound outcomes and scheduled recurrence review. Patients who do not proceed to surgery should be characterized separately to clarify selection. Follow-up should include the date and method of recurrence detection, death and last contact, allowing a genuine time-to-event analysis with competing events where appropriate. These are specific data requirements revealed by the present reanalysis, not evidence that any particular intervention has been shown to improve outcomes.

Before expanding the cohort, the immediate priority would be a source-to-release reconciliation by the data custodian. This would map the published eligibility sequence to an anonymized record inventory, explain whether the extra released rows were excluded for a clinical reason, confirm the direction and meaning of all outcome codes, and document the intended follow-up window. A corrected release should carry a new version identifier and a description of the changes, after which the present scripts could quantify their consequences. If recurrence ascertainment can be linked to dated visits, the clinical question could be reformulated prospectively around an explicit time horizon and competing events. Until then, additional model complexity would not repair the missing temporal information. The present package is useful precisely because the computations and their limits can be revisited when that information becomes available.

Conclusions

In this released Riyadh surgical cohort, the association between preoperative BMI and recorded postoperative hernia recurrence was imprecise and sensitive to the analytic population and adjustment set. The data do not establish a protective BMI association, a negligible risk difference or a defensible BMI

cutoff for surgery. The reproducible analysis provides a bounded local estimate while identifying the clinical and follow-up information needed for a more decisive perioperative study.

Declarations

Ethics and consent: Secondary analysis of an openly deposited de-identified dataset; no patients were contacted. The underlying study's ethics documentation is not reproduced in this manuscript.

Data and code availability: The source workbook is openly deposited on Figshare under CC0 [2]. The accompanying package contains an unchanged copy, its hash, an analytic CSV, the frozen analysis plan, executable analysis and verification code, machine-readable locked results, and figure-source data. No patient identifier is present in the released workbook.

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Conflicts of interest: The authors declare no competing interests.

Generative AI disclosure: Artificial intelligence was not used in the preparation of this manuscript.

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